From Dan Goodman - February 24, 1999

Here are some brief comments on the communications from MMC and IATTC.

MMC

The only issue raised in the MMC correspondence which seems to require some comment from me is the matter of the Congressional intent relative to an implicit standard that Rmax should not be "significantly" lower than 4%. Like Paul, I think we can readily incorporate this standard into our existing analysis; but rather than describe this as an alternative analysis, I think we might best add this standard to our present list of criteria in the decision analysis, and address this criterion in the same overall Bayesian analysis that Paul is already running.

This could take the form of amending Section V of the Jan 24 Decision Framework document, appending a criterion (4) to the present list of 3 criteria in the proposed decision rule. This might read:

(4) A criterion based on risk of depressing the achieved Rmax below 4%

"There must be less than Z% probability that the sum of the reported post-1991 kill rate plus the estimate of the post-1991 unreported mortality exceeds the estimate of the quantity (Rmax-0.04)."

Technically, this new criterion is a worthwhile addition to the decision framework that we are developing, because it addresses the reasonable question whether there is some adverse effect that was operating at or above a level of concern even before 1991. By contrast, the proposed the decision rule we have drafted with respect to criteria (1)-(3) is driven entirely by an increase in adverse effect after 1991, implicitly assuming that the unreported mortality effect pre-1991 was zero, and presuming that the Rmax pre-1991 was acceptable regardless of its magnitude. The new criterion allows us to ask whether the estimate of Rmax (which is driven by the pre-1991 data inputs) is itself indicative that something is causing a problem for the population. The new criterion could determine that there might be a problem even under conditions of zero reported kill, and zero post-1991 unreported mortality as defined by the analysis.

The tolerance level Z, like the other tolerance levels we are using, is a policy matter, that should be proposed in a consensus setting (such as the previous workshops we have held to develop this framework). We might start the discussion by proposing a value 5%, and see where this leads.

The decision quantity for this criterion,

[(Rmax-0.04)-((reported post91 kill rate)+(unreported post91 mortality))]

falls out directly from the joint posterior distribution in the analysis that Paul Wade has conducted.

IATTC

As I understand the letters from the IATTC, their concerns with respect to the quantitative analysis and proposed decision framework can be lumped into 3 main concerns. I would summarize these concerns as:

1) The proposed tolerance levels in the proposed decision rule are protective.

2) There are many possible confounding factors that bear on attribution of causation of "failure to recover."

3) The precisions of both the RV absolute abundance estimates by year, and the TVOD relative abundance estimates by year, could in some sense be characterized as "large," and this has implications for their use in estimating population trends that in turn are used for estimating the decision quantities such as Rmax and unreported post-1991 mortality.

I will address these in order.

1) The proposed tolerance levels in the proposed decision rule are protective.

Indeed, the proposed tolerance levels in the proposed decision rule are protective. This is appropriate for an agency charged with implementing the provisions of the MMPA and the ESA as they apply to these dolphin populations. The rationale for the particular levels of precautionary margin chosen is described in the draft Decision Framework document.

2) There are many possible confounding factors that bear on attribution of causation of "failure to recover."

Indeed, there are many possible confounding factors that bear on attribution of causation of "failure to recover." These are acknowledged in the draft Decision Framework document. The proposed

decision rule for the March 99 decision, as proposed in this Decision Framework document, does not attempt to attribute causation.

3) The precisions of both the RV absolute abundance estimates by year, and the TVOD relative abundance estimates by year, could in some sense be characterized as "large," and this has implications for their use in estimating population trends that in turn are used for estimating the decision quantities such as Rmax and unreported post-1991 mortality.

Indeed, the precisions of both the RV absolute abundance estimates by year, and the TVOD relative abundance estimates by year, are both large enough that they need to be taken into account in the analysis. Fortunately, the analysis being carried out by Paul Wade does take the magnitude of these respective precisions into account, year by year, and data source by data source. Furthermore, the analysis does not indicate any inconsistency between the precision values used and the resulting fit of the data to a population trajectory.

Nevertheless, the two facts, (a) that the precisions are being taken into account, and (b) that the resulting fit to the data looks reasonable, do not dismiss all questions about those precisions. The analysis Paul is conducting accepts at face value the published estimates of the precisions of these inputs as calculated by the respective institutions. In both cases--RV absolute abundance estimates, and TVOD relative abundance estimates--the estimation procedures are very involved. There might well be merit to deeper analysis of how those published confidence intervals have been calculated, and whether those methods are fully consistent with use of the computed coefficients of variation in a likelihood calculation in a subsequent Bayesian analysis. However, it must be understood that such a deeper analysis would be a demanding and time consuming new research undertaking that cannot be accomplished on a time frame consistent with the deadlines for a March 99 decision.

The analysis now being carried out by Paul goes about as far as is possible using published, peer reviewed data, and published, peer reviewed modeling and statistical procedures. Going deeper into questions about the data and the methods would require breaking new ground, and that, in turn, would require considerably more time, both for the analysis itself and for the peer review and publication process to establish scientific acceptance.

DG